

Foote, of the Geological Survey of India, near Madras. These were all of the ruder forms, so well known as characterizing the flint implements which have excited so much attention within the last few years in Europe. They were all formed of dense semivitreous quartzite—a rock which occurred in immense abundance in districts close to where these implements had been found, and which formed a very good substitute for the flints of north Europe. This was the first instance in which, so far as he knew, such stone implements had been found in India *in situ*. True celts, of a totally different type and much higher finish, and in every respect identical with those found in Scotland and Ireland, had been met with in large numbers in Central India, but never actually imbedded in any deposits. They were invariably found under holy trees or in sacred places, and were objects of reverence and worship to the people, who could give no information as to the source from which they had been originally gathered together. A single and very doubtful fragment of a stone implement had been found by Mr. W. Theobald, jun., in examining the deposits of the Gangetic plains near the Soane river. This occurred in the Kunkurry clay of that district; but, with this exception, he was not aware of any stone implements of any kind having previously been noticed *in situ* anywhere in India. Those now on the table had been collected partly by himself, from a ferruginous lateritic gravel-bed, which extended irregularly over a very large area west of Madras. In places this was at least 15 feet below the surface, cut through by streams, and in one such place, from which some of the specimens on the table were procured, there stood an old ruined pagoda on the surface, evidencing that, at least at the time of its construction, that surface was a permanent one. This bed of gravel was in many places exposed on the surface, and had been partially denuded; and it was in such localities, where these implements had been washed out of the bed, and lay strewed on the surface, that they were found most plentifully.

Mr. Oldham remarked on the great interest attaching to such a discovery, and on the probable age of the deposit in which they occurred. Another point of interest connected with the history of such implements was the remarkable fact that while, scattered in abundance over the districts where they occurred, were noble remains of what would by many be called Druidical character—circles of large standing stones, cromlechs, kistvaens, often of large size and well preserved, all of which were traditionally referred to the Karumbers, a race of which there still existed traces in the hills, still all the weapons and implements of every kind found in these stone structures were invariably of iron. No information whatever regarding these stone implements could be obtained from the peasantry, who had been quite unaware of their existence.—*Journ. of the Asiatic Society of Bengal*, No. I. (1864).

*On the Present State of Malacological Nomenclature.*

By PHILIP P. CARPENTER, B.A., Ph.D.

At a time when the British Association are about to revise their

“Rules,” it may be worth while to collect the experience of workers in different branches of science.

The nomenclature of Mollusca is not only in a most unsettled condition, but there seems no hope of bringing leading writers to an agreement on any first principles. Dr. Gray, whose contributions to malacology are second to none, and whose position at the head of the department in the British Museum would alone give the greatest weight to his example, has systematically ignored the principles on which the British Association Rules are based. The Messrs. Adams in England, Mörch in Copenhagen, many of the German and most of the rising American naturalists take the same course. In France the influence of Lamarck has restrained the modern antiquarian innovation.

Existing writers may be divided into two classes—(1) those who profess the absolute law of priority, and (2) those who accept it with limitations.

The advocates of “mere priority” claim that their rule is the only one which admits of fixed application. It is granted that, if limitations are once allowed, there will be differences of opinion as to their amount: but does the refusal of limitations produce uniformity? Putting aside the variations of opinion as to the greater or less division of genera, how can authors be brought to agree as to wherein the naming of a form consists? Those who compare Dr. Gray’s ‘Guide’ with Adams’ ‘Genera,’ or Dr. Gray’s generic names at one date with his names at another, will find that the mere-priority rule is thoroughly uncertain in its application, principally in consequence of the very loose definitions, and probably loose ideas, of the early writers. A modern author *thinks* that Klein or Link meant by a certain name a genus existing in his own mind, which he accordingly calls *TALIS*, *Klein*. But a second author thinks (and is quite sure he is right in thinking) that *TALIS*, *Klein*, means what is now considered a different genus, and alters the first author’s series of names accordingly. Perhaps Klein meant neither the first, nor the second, nor both; but had a vague idea which it is now only confusing to endeavour to reproduce. The mere-priority writers often judge of the old authors by their types or figures; but even the Linnean genera cannot thus be understood, and many authors place their typical species in the middle of the series.

Once more, among the mere-priority writers, some accept a name only if published with description or figure; others, if the name be *printed* in a list or catalogue; others, if the name be *written* in a public, and others, even in a private collection. But perhaps the namer has only spoken the name, or merely thought it; according to the strictest law of priority, might not even these claim precedence?

If the principle of limitation be once allowed, questions of detail can be debated and settled with tolerable ease; and if one author calls his species *Grayi*, another *grayi*, and a third *Grayana*, we all know what is meant, and that may suffice. But if a modern author quotes a *Cyclus*, a *Capsa*, or a *Siliquaria*, who knows what is meant?

Nomenclature clearly is for USE, not for honour or fancy. That is the best which (1) expresses what it means, and (2) cannot mean

anything else. That moreover is *publication*, in the highest sense, which is found to be in universal use. If in property there is a statute of limitations, and a given number of years' undisturbed possession is tantamount to a right, is there not the same reason for limiting property in a name? Why should not long-accepted Lamarckian names be regarded as much sacred as are considered those of Linnæus?

If such are the difficulties of settling the language of the past, not much less are those of the present. In old times a *Buccinum*, a *Bulla*, a *Mya*, meant almost anything. In Lamarckian times, a *Chiton*, a *Cerithium*, a *Pleurotoma* meant what would now be called a family. If a writer describes under these genera, we know at least in what large division to search for his species. But if he describes a *Rissoa*, a *Modelia*, a *Truncatella*, we have a right to suppose he means what he says, and cannot be expected to look for his species in another suborder. If his *Rissoa* proves to be a *Chrysallida*, his *Modelia* a *Lacuna*, and his *Truncatella* a *Hydrobia*, is he entitled to priority if his successor, anxiously desirous to make out his species, has been compelled through necessary ignorance to redescribe? Very often neither the diagnosis nor the figure represent the real shell. If an author, seeing one object before his eyes, which he calls his type, describes another, and sends a third to the Cumingian collection to represent his species, for which must his name stand? Does it not really belong to the *idea in his own mind* which is embodied in his diagnosis, or (if an artist) in his figure, rather than to the shell which is not represented by either one or the other? A truthful name therefore, even though second or third in time, may be more *useful* to science than a false one given first.

Space only allows us to point out one more difficulty in modern nomenclature. In old times a species (and even a genus) was supposed to be clearly defined. The Darwinian theory offers a satisfactory explanation of some facts in nature, to many who are not prepared fully to accept it. Every worker among large series finds forms which may *or may not* prove conspecific with others, the evidence not being as yet conclusive; he describes these as doubtful varieties. Does not the careful naming and description of a *form* establish a claim for priority, whether by succeeding writers that form be regarded as a variety, a species, or even a genus?

It depends much on habit of mind whether authors prefer to work by large or by minute divisions. When we speak of *Callista undulata*, it is a matter of little consequence whether *Callista* be regarded as a subgenus of *Cytherea* or a separate genus, whether *undulata* be regarded as a variety of *planulata* or a distinct species. What is of consequence is, that all the scientific world should have the means of knowing at once what group of forms are included in *Callista*, what kind of individuals in *undulata*. First, then, we need accurate descriptions, then these descriptions condensed into useful nomenclature. Science being a republic, there is no chance of even the forthcoming Rules of the British Association being considered obligatory. But many persons who will not allow themselves to be ruled, against what they consider a principle, may yet be brought to



make concessions. The Academicians had great success in fixing the French language. Why should there not be a congress of malacological authors\*, undertaken in a spirit of mutual respect, who should fix such names to existing genera as in each case should prove most *useful* because most widely or easily understood? If travelling is dear, postage is cheap. At present, to teach the science is almost hopeless: to labour in it is fraught to each worker with the unnecessary sacrifice of most valuable time. All considerations of supposed honour to individuals, whether dead or living (which often is equivalent to dishonour, because evidence of work done badly), ought to give way to the manifest benefit, we might almost say necessity, of using words to express a given meaning in science, as we do in common life.

*On Hermaphrodite Bees.* By PROFESSOR VON SIEBOLD.

An intelligent apiarian at Constance, M. Engster, was struck, four years ago, by the abundant production of hermaphrodite bees in a Dzierzon hive inhabited by Italian bees. Similar monstrosities have already been occasionally mentioned. At the commencement of this century a schoolmaster of the name of Lukas, described them under the name of "Sting-drones" (*Stacheldrohnen*); but his discovery was regarded as fabulous, and it is only of late that MM. Dœnhoff and Menzel have recognized some hermaphrodite bees. It is fortunate that so competent an observer as Professor Siebold has been able to investigate the abundant supply of these monstrosities furnished by M. Engster's hive, as Dœnhoff ascribes perfect male generative organs to the individuals dissected by him, whilst Menzel always found those organs atrophied.

Professor Siebold differs from both his predecessors, having found among the hermaphrodite bees a mixture of sexual characters not only in those organs which are not directly connected with reproduction, but also in the generative apparatus itself. The mixture of these characters varies greatly in different individuals. It is manifested sometimes only in the anterior, sometimes only in the posterior part of the body; sometimes in all parts of the body, and sometimes only in a few organs. Some individuals present the characters of a drone on the right side, and on the left those of a worker; others are drones in front, and workers behind. The intercalation of different sexual parts sometimes takes place very curiously. Lastly, in some individuals the hermaphroditism is limited to the borrowing of the characters of a single organ (jaws, eyes, antennæ, or feet) from the other sex.

The internal organization presents anomalies of the same kind, but the hermaphroditism of the generative organs is rarely related to that of the external parts. The sting, with its vesicle and poison-gland, is well developed in the hermaphrodites with the abdomen of the worker; it is soft and deformed in those in which the abdomen resembles that of the drone. The oviduct is often furnished with

\* This was proposed, for naturalists in general, by Dr. Stimpson: *vide* 'Silliman's Journal' for March 1860, pp. 289-293.